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Rare B-Decays: Experimental Prospects and Problems*

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Rare B-Decays: Experimental Prospects and Problems*

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Abstract

We discuss the future prospects and experimental requirements for generic b-physics, with an emphasis on hadroproduction and on observation of CP violation.

I. Preliminary Remarks

My interest in B-physics is conditioned by my presence at Fermilab. I carry a special bias toward the long range potential contribution to this subject of experiments utilizing hadron-hadron collisions. Every minute that the Tevatron machine is delivering beam to experiments, more $b\bar{b}$ pairs are produced than will be produced in the history of SLC, LEP, CESR, and DORIS combined. Regrettably these b's reside in the interior of the beam-dumps, quite remote from direct access. But this still remains a spur toward working hard in exploiting this potential resource as much as possible.

In this survey of experimental prospects, I will not emphasize the electron-positron collider potentialities. This is not out of disinterest, but simply because the existing lore is quite extensive, and better covered by others.

II. What We Want To Know

I shall only briefly enumerate the obvious topics of interest in b-physics:

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1. Production dynamics

This is a more reliable QCD test than charm photoproduction. The situation in e^+e^- collisions is already quite good. Charm production dynamics is reasonably described¹ in terms of QCD photon-gluon fusion mechanisms, although there is a normalization issue. This leaves hadroproduction as the most uncertain case. Important issues to me include:

- i) Leading-particle effects: Relative to charm hadroproduction these diminish in importance as $(m_c/m_p)^{-1}$, but are important to establish (or rule out).
- ii) Normalization: There is a rather big uncertainty² from choice of b-quark effective mass (5.0 ± 0.3 GeV seems to be the chosen range), choice of gluon distribution functions, and an unknown "K-factor".
- iii) A Dependence: Are there any $A^{2/3}$ effects, especially for forward production?
- iv) Baryon yield: The large yield of $\Xi_c^+(\text{usc})$ baryons seen³ by Fermilab experiment E400 (central production with 600 GeV incident neutrons) suggests b-baryon production may likewise be quite substantial. I suspect everyone (even the e^+e^- community?) may underestimate the importance of baryons.

2. Spectroscopy

All species need independent study and identification. B_u and B_d lifetimes should be separated. It is not satisfactory to claim a measurement of $B_s - \bar{B}_s$ mixing without having yet discovered the particles themselves. The

various species of b-baryons are useful as probes of bound-state properties. An excited $B_d^* \rightarrow B_d + \pi^+$ would be invaluable tag for mixing and CP studies. Or how about $B_s^* \rightarrow B_d + K^+$? or $\Sigma_b^* \rightarrow \bar{B}_d + p$? Perhaps some species of B (i.e. $\bar{b}q$) may bind weakly to (or resonate with) some species of baryon a la deuterium. While these possibilities are somewhat unlikely, the search ought to be carried out (the same can, of course, be said for charm).

3. Decay Phenomena

To understand Cabibbo theory well, a large number of strange meson and baryon decay modes needed exhaustive study. The same is now true for heavy flavors: a large data base of charm and bottom decay channels is needed to reliably extract the Kobayashi-Maskawa matrix elements. The validity of the spectator/factorization model of B decays needs more testing. Optimal "signature" decay modes of B mesons and baryons need to be established. Final states containing $\tau\nu_\tau$ would be splendid to see. And final states containing ψ are of course extremely interesting.

4. Mixing

The recent ARGUS results⁴ on B_d mixing, assuming they are confirmed, evidently have revolutionary consequences. Much of the theoretical work, especially the studies on CP violation, now needs major revision and reconsideration. It is a good example of how acquisition of a few simple facts can greatly change experimental strategies for the future. For another example, our prejudices regarding m_t may now change, with a tilt toward a much heavier top quark.⁵ If $m_t > m_w$, even the Higgs-search strategies for $m_H < 2m_w$ change. ($H^0 \rightarrow b\bar{b}$ dominates; $H^0 \rightarrow \mu^+\mu^-$ has a nontrivial branching ratio, etc.).

Also $B_s - \bar{B}_s$ mixing may be expected to be very rapid; several oscillations per mean life is quite reasonable. Measurement of $(\Delta m/\Gamma)$ for the B_s may therefore require very good time-resolution, and result in quite spectacular data.

5. Rare Decay Searches

The long lifetime and relatively large b-quark mass implies a special sensitivity to new physics in decay modes with eventually detectable branching ratio, e.g. $B \rightarrow \mu e$ hadrons, $b \rightarrow s l \bar{l}$, etc. Other rare modes, e.g. $B_u \rightarrow \tau \nu_\tau$, are interesting in their own right.

6. CP Violation

The large CP-violation effects in the B-system which are anticipated within the standard model⁶ make this an irresistible experimental goal. The best experimental strategy for seeing CP violation is as yet unclear. It should be emphasized that a firm experimental command of the preceding five topics is probably a prerequisite to succeeding in the detection of CP violation in the B system. I suspect we are a very long way from success in that endeavor.

7. Bottom-quark Engineering

The long-range frontier of very high mass-scales is undoubtedly dominated by multi-jet spectroscopy. Detection of leptons, photons, and quark and gluon jets are therefore of highest priority. But close behind is identification of the flavor content of hadron jets. QCD bremsstrahlung gluons will be a curse in this field, and sorting out interesting quark-induced jets (especially heavy-quark jets) from the large gluon-jet background will be a challenge of increasing importance as the energy scale continues to go up. In the long run, this may technically be the hardest problem for heavy flavor

experimentation. Successful, efficient isolation of $W \rightarrow c\bar{b}$ and/or $Z^0 \rightarrow c\bar{c}$, $b\bar{b}$ signals would be a landmark accomplishment.

In any case, the study of generic b-physics has as its most important long-range goal the measurement of CP violation in the B system. Prior to the ARGUS results, my guess of the required number of produced $b\bar{b}$ pairs per experiment for doing this was at least 10^9 . This is reasonably consistent with what is found^{7,8} in the paper studies (mostly by theorists!). The back-of-the-envelope argument goes as follows:

- 1) A specific nonleptonic decay chain appears to be needed:

$$\begin{array}{l} B \rightarrow DX \quad (1\%) \\ \quad \downarrow \\ \quad Y \quad (3\%) \end{array}$$

The percentages measure typical branching ratios. Alternative options such as $B \rightarrow$ uncharmed, $B \rightarrow \phi X$, or $B \rightarrow$ charm-anticharm lead to net branching fractions no larger.

- 2) The neutral B must identified at birth as B or \bar{B} , and as strange or non-strange. An efficiency of more than 10% per $b\bar{b}$ event seems optimistic.
- 3) Good statistics is needed to be convincing even if the effect is gross. I take the number of events per decay channel of interest to be $\gtrsim 10^3$.
- 4) Prior to the ARGUS result, either a first-forbidden B_s decay or a delicate effect in B_d decay (due to the small mixing expected) was required. I gave a factor 30 derating here, a factor perhaps no longer operative.

So my bottom line of $\geq 10^9$ $b\bar{b}$ produced per experiment might shrink to a mere 3×10^7 by the recent results. And there is of course a great uncertainty in this estimate. The number might be reduced by a clever, optimal strategy. On the other hand, little allowance has been made for the invariable losses from incomplete acceptance, detection inefficiencies, etc. etc. which occur in real experiments as opposed to paper studies conducted by theorists. The real bottom line is, in order to have a chance of success with CP, to throw deep.

II. Prospects and Limitations of $e^+e^- \rightarrow b\bar{b}$

Two distinct alternatives for $b\bar{b}$ physics are threshold e^+e^- machines, e.g. CESR or DORIS, or Z^0 factories. Other energies seem to me to be clearly disadvantageous:

A. Threshold Machines

At the $\Upsilon(4S)$, $\sigma(b\bar{b})$ is $\sim 10^{-33} \text{cm}^2$. If we take $\int L dt / \text{experiment}$ to be 10^{39}cm^{-2} , we obtain 10^6 $b\bar{b}$ produced. The advantages of working at threshold are well-known; disadvantages for generic studies are that B_s and baryons are difficult, and no lifetime information can be gleaned. This might be mitigated somewhat if, as has been discussed, asymmetric collisions ($2 \times 12 \text{ GeV}$?) could be accommodated in some future machine.

B. Z^0 Factories

At the Z , $\sigma(b\bar{b}) \sim 5 \times 10^{-33} \text{cm}^2$. With $\int L dt / \text{experiment} \sim 2 \times 10^{38} \text{cm}^{-2}$, one obtains again 10^6 $b\bar{b}$ per experiment. In this case, the b 's move relativistically and microvertex detection can be used. But technical problems of a big beam pipe and barrel geometry remain. The smaller SLC beam pipe and beam polarization are an advantage there; the bigger question at SLAC may be ultimate luminosity: is $2 \times 10^{38} \text{cm}^{-2} / \text{experiment}$ a reasonable estimate?

For generic studies of B_s and baryons, the Z^0 factories should have an advantage. Also LEP + SLD comprise an array of very sophisticated and expensive detectors; this ought somehow to be of importance.

Evidently the main limitation for e^+e^- colliders is simply luminosity; an order of magnitude improvement would produce a great deal of physics. But that may still fall short of what is needed for CP physics.

III. External Photon and Hadron Beams

There are many initiatives for heavy flavor physics in this category which are underway. Before cataloguing these, however, it may be worthwhile reviewing some of the necessary conditions for doing $b\bar{b}$ physics via this technique:

A. Spectrometer Properties

- 1.) Because the $b\bar{b}$ signal is so small ($\lesssim 10^{-6}$ of all events in hadron-nucleus collisions; $\lesssim 3 \times 10^{-5}$ of all hadron photoproduction events), high rates are essential. For hadroproduction, one must deal with $10^{6 \pm 1}$ interacting beam particles/sec. Of these, $10^{1 \pm 1}$ /sec can be expected to be recorded on tape, implying a selectivity of the data acquisition system of one in $\geq 10^{5 \pm 1}$ events.
- 2.) Isolation of the secondary decay vertex is essential. The success with silicon microstrip detectors is evidence that this can be done. Other techniques (e.g. high resolution "straw" detectors, optical fibers, CCD's) are also candidates, as are "active targets"; i.e. detectors for which the secondary decay vertex is contained within (These include

high resolution streamer chambers, emulsion, and scintillating glass fiber targets).

- 3.) The usual criteria for a good spectrometer (broad acceptance, accurate charged particle resolution, good electromagnetic calorimetry and muon identification, Cerenkov identification, etc., etc.) apply with special force because of the heavy demands of high rate and low signal/noise ratio.

B. Event Selection Criteria

It must still be learned how best to attain the large rejection factor of 10^5 . The ideas extant include

- 1.) High P_T lepton ($> 1-2$ GeV): While this strategy worked poorly for charm, there is good reason⁹ to believe it is much better for bottom, because of the larger b-quark mass.
- 2.) ψ Trigger: The decay $b \rightarrow \psi s$ proceeds with a 1% branching ratio. The signature is evidently excellent, especially when combined with the secondary-vertex information. It also is encouraging that CP violating effects are large in some channels such as $B_d \rightarrow \psi K_s$. My personal favorite is the Cabibbo forbidden mode $B_d \rightarrow \psi \pi^+ \pi^-$ (branching ratio $\sim 10^{-4}$), which has splendid signature and intrinsic properties, including CP violating effects at the level of ψK_s .
- 3.) Secondary-vertex tag: The goal here is to trigger on events with evidence of a secondary vertex, using only the data from the front end microvertex tracking system. There are many ideas on doing this¹⁰ but as yet it seems not quite state-of-the-art.

- 4.) Charm tag: Perhaps one might preselect $D^+ + K^- \pi^+ \pi^+$ or $D^0 + K^- \pi^+$ candidates by an online trigger processor, and thereby work back toward the B's. This seems hard.
- 5.) Several high p_T hadrons: This may be especially helpful in isolating low-multiplicity B-decays (the ones one is most interested in anyway). This approach will be tried¹¹ at CERN in the next year or two.

It seems probable to me that a combination of these strategies may be needed to get the desired rejection factor of $\gtrsim 10^5$.

C. Tagging the Initial B

The time dependence of neutral B-decays provides an especially sensitive measure of CP violation. For example for final states which are CP eigenstates

$$\begin{aligned}\Gamma(B \rightarrow f) &= \Gamma_0 [1 + (\text{Im } \lambda) \sin \Delta m t] e^{-\Gamma t} \\ \Gamma(\bar{B} \rightarrow f) &= \Gamma_0 [1 - (\text{Im } \lambda) \sin \Delta m t] e^{-\Gamma t}\end{aligned}$$

where λ is of modulus unity and is real if CP is conserved. To see an effect, one must know whether the meson at birth was B or \bar{B} . Strategies for doing this⁸ include

1. Finding an associated B_u/\bar{B}_u or $\Lambda_b/\bar{\Lambda}_b$.
2. Finding a resonance, e.g. $B^* \rightarrow B\pi$ or BK , analogous to the oft-used D^* . However for the B-system, the B^* has to be a p-wave excitation.
3. Finding an associated B^0 via e.g. semileptonic decay, and correcting for its mixing. This is tricky, especially if B^0 is a B_s/\bar{B}_s .
4. Using only leading B's ($x_F \gtrsim 0.2-0.3$?) where one may expect a \bar{B}/B production ratio different from unity because of hadronization effects.

This has the advantage of an inclusive measurement but throws away a lot of B's.

The tagging problem and the related problem of resolving B_s and B_d final states is a heavy demand on experimental designs and needs careful attention.

D. What is Happening Now?

1. CERN SPS: To my knowledge only one directly observed hadroproduced $B\bar{B}$ pair has been reported;¹² more may be on the way. This was achieved via emulsion and muon trigger; a follow-up measurement in closed geometry¹³ has yielded a coarse estimate of B production by pions at 300 GeV.

Other activities at CERN center around the Omega Spectrometer. This year will see an attempt¹⁰ (experiment WA82) to trigger on secondary vertices with a silicon microstrip front end. Next year will feature¹¹ a scintillating glass fiber target, with fibers aligned along the beam (Fig. 1). The readout is from upstream, using image-intensifiers and CCD's. The trigger will be 3 high- p_T hadrons seen in the downstream spectrometer.

2. Fermilab:¹⁴ The very successful¹⁵ charm photoproduction experiment E691 will be followed by a charm hadroproduction experiment (E769) with incident pions and kaons. However, the low primary energy of ~ 250 GeV and loose trigger does not make this a strong $B\bar{B}$ experiment. But it does have the capability of seeing some $b\bar{b}$ events. This is also true of a new photoproduction experiment, E687. Also, an experiment (E653) with emulsion target, a silicon

microstrip tracker, followed by an open-geometry spectrometer and muon trigger, has had one run and will continue this year with high energy pions incident. One may expect some B events from this experiment also.

Another experiment (E690) scheduled to begin in 1988, will study charm production in target-dissociation processes. It features a very sophisticated pipeline processor capable of full reconstruction of complex events on-line at high rate. This system has been developed during a BNL experiment, and now appears to be essentially ready to go. This experiment has much to say about the capability of on-line event reconstruction, but it does not directly address B-physics in its present incarnation.

A new initiative with a fresh approval (E771), is a high-rate experiment which will trigger on $B \rightarrow \phi X \rightarrow \mu^+ \mu^- X$ and observe the secondary decay vertex with a silicon microstrip system. It should have a preliminary run in 1988. Meanwhile an experiment (E706) designed for direct-photon physics has been merged with another (E672) designed for study of hadrons produced in association with Drell-Yan dileptons. Because E706 has a silicon microstrip detector, this provides a serendipitous opportunity to try for this kind of physics already this year.

My personal view of all this is that at best the main result of this round of experiments will be to map out B-production characteristics. The yield will probably be too small for spectroscopy, mixing studies, etc. However, useful

lifetime information may emerge, along with initial data on Λ_b and B_s . The $b \rightarrow \psi$ s approach is also very strong, and has a great deal of promise. However, it may well be that hadroproduction of $b\bar{b}$ today is where hadroproduction of $c\bar{c}$ was a decade ago. In any event, the aforementioned round of experiments should do a splendid job on charm physics, fully competitive with and complementary to what is being done in e^+e^- collisions. The topics should include rare decays, baryon physics, $D - \bar{D}$ mixing, and perhaps double-charm physics, e.g. Ξ_{cc}^{++} (ccu) production.

IV. Generic Bottom-Physics in Hadron-Hadron Colliders

It is a difficult but not hopeless business to find secondary vertices in the high rate environment characteristic of a hadron-hadron collider. The presence of a beam-pipe, intense circulating beam, barrel geometry, and a large interaction-region volume exacerbates the problems. Furthermore the typical "4 π " detector which typically surrounds such collision regions is optimized for high- p_T jet/lepton/photon physics, not low- p_T "minimum-bias" final states characteristic of generic $b\bar{b}$ production. Initiatives and plans for UA1, CDF, and DØ can be expected to emphasize physics goals other than generic $b\bar{b}$ production.

Beyond these initiatives exists an effort¹⁶ to look at forward heavy-flavor production upstream/downstream of UA2. And at the Fermilab collider there are nascent ideas on generic $b\bar{b}$ experiments at 2 TeV in the center-of-mass. The $b\bar{b}$ yields may be as high as 3×10^{-4} per collision, or $\gtrsim 20 \text{ sec}^{-1}$ at $L = 10^{30} \text{ cm}^{-2} \text{ sec}^{-1}$. Thus in principle the yields per experiment suffice to enter the domain of CP violating effects. However, the problems are daunting.

The products of B decays are found over a large range of rapidities; the most useful B's are those which move relativistically in the laboratory frame. I estimate a good angular acceptance to be $20 \text{ mrad} \lesssim |\theta| \lesssim 800 \text{ mrad}$, implying emphasis on "planar" detector geometry similar to fixed-target architectures. However, much is gained if the "barrel" region is also covered. One of the suggested designs¹⁷ is shown in Fig. 2; it is "planar" and covers angles up to $\sim 300 \text{ mrad}$. The other,¹⁸ produced by N. Lockyer and P. Karchin, puts more emphasis on the central "barrel" region. Both use a great deal of silicon tracking along with a transverse dipole magnetic field, and triggering via high- p_T electrons.

One of the biggest problems of doing this physics at Fermilab, other than support, is where to do it. Finding an independent collision region is problematic. At present the leading option is AØ, the locale of injection from the booster and of extraction to fixed-target experiments. Considerable work has to be done in order to determine whether this is a viable option.

The most detailed documentation⁸ for generic $b\bar{b}$ collider experimentation exists for the SSC. At SSC energies, at least one event in 10^3 should contain $b\bar{b}$ pairs. Given a luminosity $\lesssim 10^{32} \text{ cm}^{-2} \text{ sec}^{-1}$, implying an event rate perhaps usable for open-geometry measurements, one finds a yield of at least 3×10^{10} produced $b\bar{b}$ pairs per experiment. Again the problem of digging them out is a great challenge. A sample experiment (the TASTER) is described¹⁹ by B. Cox and D. Wagoner in the 1986 Snowmass proceedings. Most $b\bar{b}$ pairs, even at the high SSC cms energy, are produced reasonably centrally (i.e. within an interval of ± 3 units of rapidity). Thus back-to-back detectors, each looking

like typical fixed-target spectrometers, are a reasonable architecture. The TASTER (Fig. 3) utilizes a high- p_T lepton and/or ψ trigger to select B candidates. Silicon planes up close are used to find the secondary vertices. It is remarkable how similar the TASTER is to Cox's fixed-target experiment E705/E771 and Reay's Tev I detector layout is to his experiment E653. This exhibits how similar the detection problems at the colliders are to those in external hadron beams. Or perhaps it is an indication of the advancing age of the proponents.

V. Summary; Some Opinions on Long Range Possibilities

Doing generic $b\bar{b}$ physics via photoproduction and hadroproduction is, to say the least, in a primitive state. Nevertheless the long range possibilities are technology-limited, not rate-limited. The numbers of $b\bar{b}$ pairs which are produced per experiment in hadron-hadron collisions, both at colliders and even in external beams, can be far in excess of what can be anticipated from any e^+e^- collider. The technology centers on (a) precise event-by-event identification of secondary vertices via microvertex techniques, (b) high rate-capability, and (c) sophisticated on-line data processing and event selection. All these technologies are now progressing rapidly, and it is not easy to project where the ultimate limitations will be.

Probably the e^+e^- colliders will remain the principal source of information on the $b\bar{b}$ system for some time. For hadroproduction experiments to truly compete, one should strive for experiments in which the number of $b\bar{b}$ produced per experiment is greatly in excess of 10^6 . This is not to say exploratory

experiments are not important. They are needed as the first step. And $\gtrsim 10^5$ produced $b\bar{b}$ per experiment implies $\gtrsim 10^8$ $c\bar{c}$ produced, far in excess of e^+e^- capabilities. Nevertheless I think the long-range goal should be kept clearly out in front.

I also suspect that the architecture and scale of detectors to do this physics may well be large compared to what is found in traditional fixed-target initiatives. My own criterion of good acceptance for a fixed-target spectrometer is that it cover a solid angle of $\gtrsim 6\pi$. By this I mean ~ 5 units of rapidity should be well covered; a typical " 4π " detector covers about 3 units. For the Tevatron fixed-target application my choice of angular range would be $5 \text{ mrad} < \theta < 500 \text{ mrad}$. This large angular range is important not only because produced b's are found in a range of rapidities, but because of the aforementioned tagging problem. This requires finding not only the products of the "trigger" b but the debris of its companion \bar{b} . The "triggering" b may be disciplined into falling within the optimal portion of the spectrometer acceptance. But that usually will not be the case for the spectator.

Another simple-minded way of judging fixed-target spectrometer architectures is to view them in a boosted reference-frame where the b-hadron of interest is produced at rest. It is easy to see that the resolution of conventional charged particle tracking elements is boost-invariant. Thus in this boosted frame, one might well demand a detector volume, acceptance, pattern-recognition capability, etc. at least as good as one would have in a generic 4π detector such as CLEO or ARGUS. The reader is invited to try this for existing detectors.

This line of argument can be reversed; if one takes CLEO or ARGUS as a suitable architecture and boosts it into the laboratory frame, it should be a suitable detector. The solenoid and barrel calorimetry is stretched by a factor $\gtrsim \gamma$ and the downstream endwall is a factor 2γ further away from the collision point.* For $\gamma \sim 20$, appropriate for Tevatron fixed-target applications, this makes a compact 2m x 2m collider-frame detector into a solenoid and barrel calorimeter $\gtrsim 40$ m in length and $\gtrsim 2$ m in diameter. To do just this would require doubling the world inventory of 4π -detector solenoids and barrel calorimeters. More efficient is to replace the calorimeter barrels with an endwall of diameter 6m, plus two more upstream walls each of the same diameter and each with a 2m diameter hole. If supplemented with three 2m diameter magnets with transverse B-fields (dipole or quadrupole; my choice is quadrupole), this would provide excellent calorimetry and magnetic analysis over all of phase space, with perhaps ample space for nondestructive particle identification as well.

I would not claim this is anywhere near optimal. But even at this extravagant level, we are talking about an investment maybe twice that of a big collider detector. And no matter how one proposes to do quality b-physics, the threshold investment is probably large. The large acceptance and very large amount of information per event that must be acquired implies a big investment in readout and data-processing electronics alone.

*The factor two originates from the fact that a fixed-target endwall boosted to the collider frame moves toward the collision point at the speed of light.

Whatever the fixed-target detector turns out to be, for collider applications one might contemplate a similar $12\pi = 2 \times 6\pi$ back-to-back pair to cover both forward and backward directions (I here assume that planar geometry dominates because most decay products of useful b-hadrons have $|\theta| < 45^\circ$). An extra 2π or 3π of solid angle for a central barrel region would also be of use.

The similarity of each detector arm with the fixed-target detectors suggests that fixed-target initiatives can be a useful bridge to the ultimate, more difficult collider case. "Collider-compatibility" of fixed-target initiatives would imply no active target and transmission of the primary beam through the detector without traversal of detection elements.

But this is more than enough speculation about detectors. I ask the reader's forgiveness for my naive indulging in such experimental design fantasies. But my main point is that maybe it is already time to think big. I cannot imagine this kind of physics going into early obsolescence. Indeed how likely is it that the new physics at the TeV mass scale will shed significant light on the origin of CP violation? The ARGUS results encourage the hope that in the long run CP violation in the b system can be observed. Maybe it is already time to throw deep.

VI. Acknowledgments

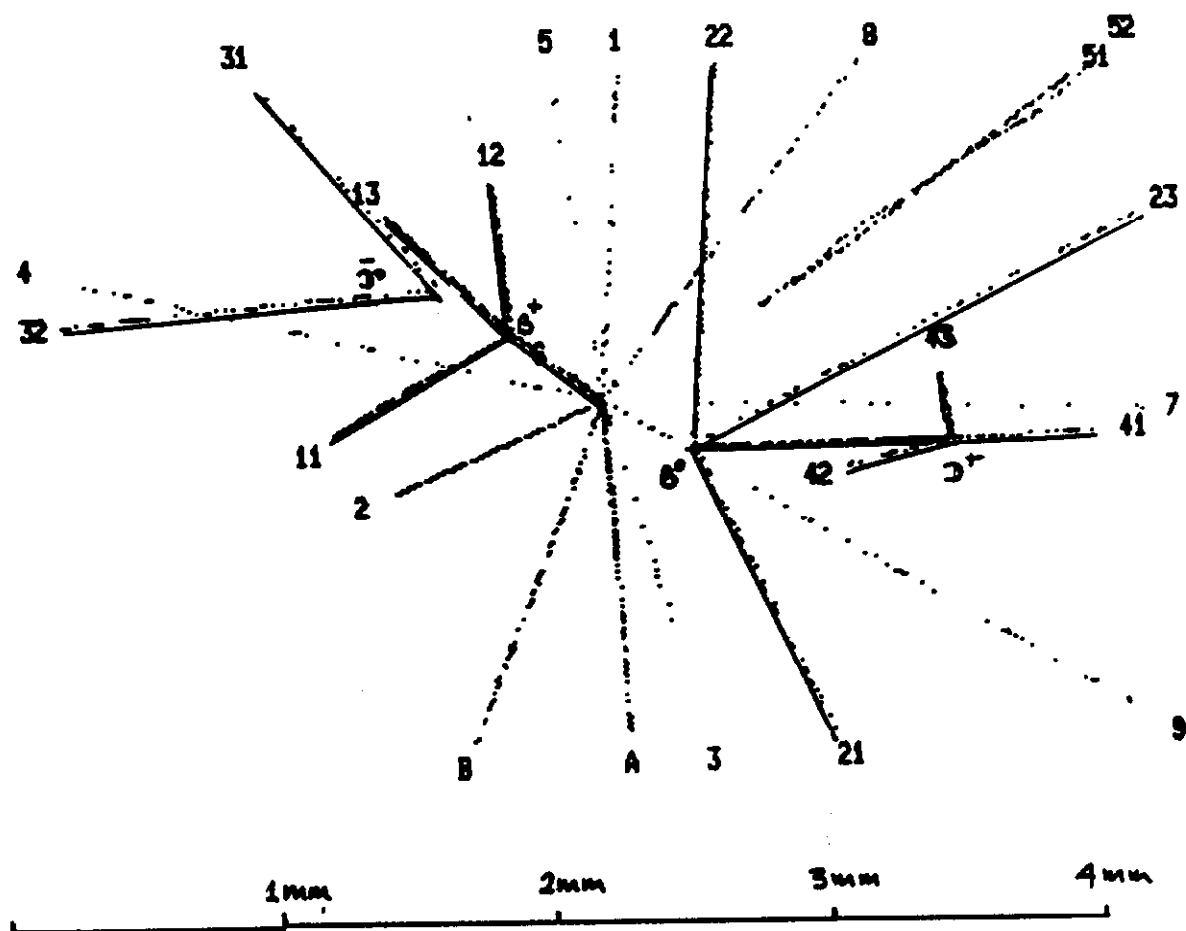
Among my many colleagues who have helped educate me in these matters, I especially want to thank B. Cox, N. Reay, D. Jovanovic, T. Nash, and J. Slaughter.

FIGURES

- Fig. 1 Simulation of a $B\bar{B}$ event as might be seen¹¹ from upstream via a scintillating glass fiber target.
- Fig. 2 A layout¹⁷ (N. Reay et. al.) of a possible Tevatron collider experiment for generic B physics.
- Fig. 3 Layout¹⁹ of an SSC experiment (the TASTER; B. Cox and D. Wagoner) for generic B-physics.

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Precision 10 μ m.
 HIT DENSITY 4 /mm.

Figure 1: Simulation of a B \bar{B} event as might be seen¹¹ from upstream via a scintillating glass fiber target.

DETECTOR LAYOUT

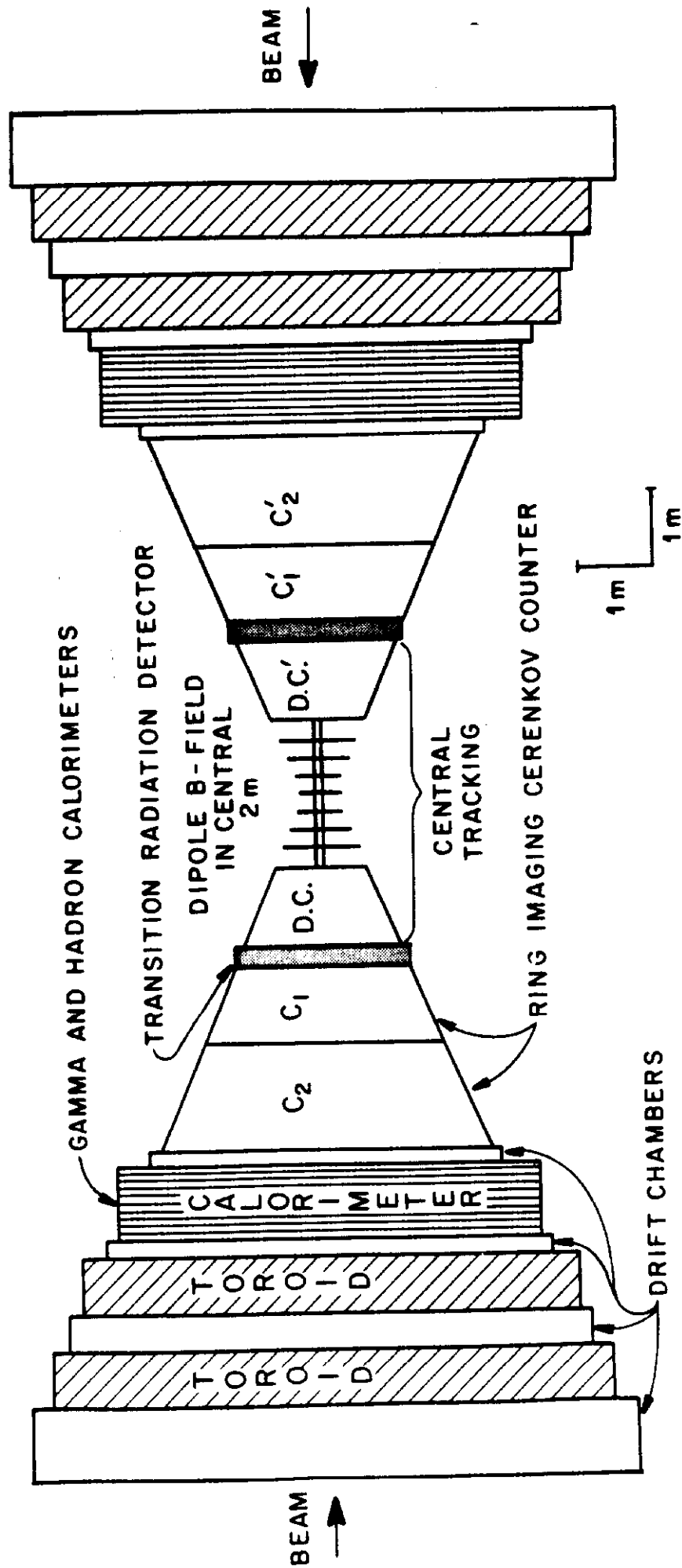
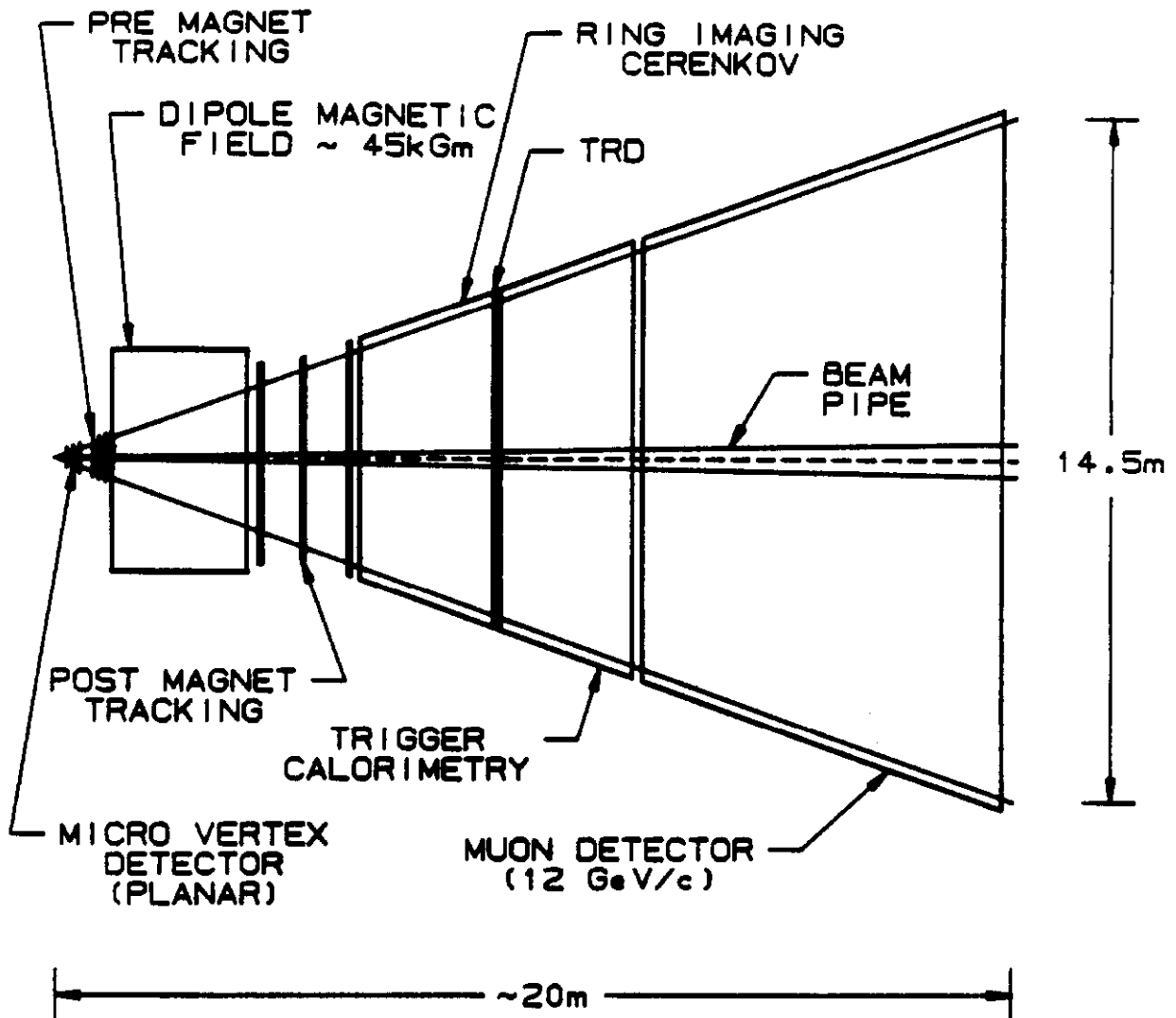


Figure 2: A layout¹⁷ (N. Reay et. al.) of a possible Tevatron collider experiment for generic B physics.

THE "TASTER"

RARE B DECAY SPECTROMETER SCHEMATIC



θ COVERAGE $1^\circ < \theta < 20^\circ$

ϕ COVERAGE 2π

Figure 3: Layout¹⁹ of an SSC experiment (the TASTER; B. Cox and D. Wagoner) for generic B-physics.